ASTROBIOLOGY
Volume 11, Number 1, 2011

DOI: 10.1089/ast.2010.1122



# The Next Phase in Our Search for Life: An Expert Discussion

***Moderator:*** Christopher P. McKay[1]
***Participants:*** Dirk Schulze-Makuch,[2] Penelope Jane Boston,[3]
Inge L. ten Kate,[4] Alfonso F. Davila,[5] and Everett Shock[6]

*This roundtable discussion was convened to discuss the ongoing planning process in planetary sciences and its relationship to Astrobiology goals. The participants were selected to represent a range of interests and backgrounds in Astrobiology and all of them have been involved in planetary missions and the use of data from planetary missions. Three questions were sent to the panel before the roundtable discussion to provide a context of initiating the discussion. These questions were:*

1. *What would you list as the most important opportunity for Astrobiology in planetary missions in the next decade?*
2. *What is the right balance between directly searching for life and investigations that focus on the physical environment providing a context for life? Does the current program strike the right balance?*
3. *The Decadal Survey process mirrors the organizational structure of NASA Headquarters (HQ). Hence, Astrobiology is included in the Planetary Decadal Survey because Astrobiology is programmatically part of the Planetary program at NASA HQ. Is this the right approach? What are other possible approaches? Does the Astrobiology Roadmap (promulgated by the NASA Astrobiology Institute) provide an important alternative? How effectively do these two processes capture real community input?*

**Christopher P. McKay:** ***Dirk, what would you list as the most important opportunity for astrobiology and planetary missions in the next decade?***

**Dirk Schulze-Makuch:** Well, there would be several possibilities, but … we are at a point where we have a very good idea about the environmental conditions on Mars and I think that we are now ready to both design and then launch a life detection mission to Mars.

**CPMcK:** That's interesting because in a recent forum article, you point to Titan as being a priority; so, if you were to list one thing, would it be a Mars mission, rather than, say, a Titan mission?

**DSM:** I feel Mars is more pressing and we are ready for a Mars life detection mission. I would like to see a Titan mission as well, but there is probably still a bit more development needed, and we have to think exactly how we want to do it. We are not at that exciting juncture with Titan where we can look for life. We first have to characterize the environment. A life detection mission for Mars is on the first place for me, and second, an elaborate Titan mission characterizing its environment.

**CPMcK:** ***Penny, how about you next?***

**Penelope Jane Boston:** I have a two-part answer to that. On one hand there is a methodological issue and I would like to see us shift our astrobiological attention—and the mission program in general—to a better balance of landed missions versus orbital missions. While orbital missions have great value for a number of different things, pushing forward the science relevant to life detection will require landed missions with increasingly sophisticated *in situ* measurements. So that is a philosophical orientation in terms of how you design the missions.

Then if I were picking my favorite body to go to—I would not necessarily couch it in those terms—I have become increasingly interested in the issue of ices in the solar system and their potential for preserving biological signals. So that actually applies to the icy terrains on Mars. It applies to icy moons around at least Jupiter and Saturn as well.

So I have started to do some work on Antarctic materials that are promising, we think, in terms of trapping microorganisms and preserving their signals. Other people, of course, are working on permafrost and so forth. Ices are an arena in which I would like to see us really aggressively advance.

**CPMcK:** ***Everett, how about you?***

**Everett Shock:** I would like to comment that one problem we face in general is that on most planets, the surface is not a very hospitable place, and yet, of course, the surface is the easiest thing for us to collect data from, either from a vast

[1]Planetary Scientist, NASA Ames Research Center, Moffett Field, California.
[2]Professor, School of Earth and Environmental Sciences, Washington State University, Pullman, Washington.
[3]Director of Cave and Karst Studies and Associate Professor of Earth & Environmental Sciences, New Mexico Institute of Mining & Technology (New Mexico Tech), Socorro, New Mexico; Associate Director of the National Cave and Karst Research Institute, Carlsbad, New Mexico.
[4]Assistant Research Scientist, NASA Goddard Space Flight Center, Greenbelt, Maryland; Goddard Earth Science and Technology Center, University of Maryland, Baltimore County, Baltimore, Maryland.
[5]Research Scientist at the SETI Institute, Mountain View, California.
[6]Professor, Chemistry & Biochemistry, Arizona State University, Tempe, Arizona.

distance or by orbit or even by landing. And I think that being able to get below the surface, at least some, is pretty much a key part to having an astrobiological perspective on these kinds of expeditions.

So unfortunately, it is a little hard to know how deep to go beneath the surface. I would think in the Mars polar region you might not have to go very deep to find a set of circumstances that is conducive to life at the present. In an icy satellite you might have a bigger problem, having to get through quite a bit of ice. But thinking now, seriously thinking about how we are going to get below the surface, even though that is the part we know the most about, is probably a key to making real advances.

**CPMcK:** Well, I think it is an interesting perspective, Everett, and we do not seem to be making much success in getting missions that go below the surface. As you say, it seems much, much harder.

**ES:** It is much harder, and it is a topic that keeps coming up. Starting now to think about how we are going to get below the surface might make it possible in a decade or so to really do something serious.

**CPMcK:** *Alfonso?*

**Alfonso F. Davila:** Well, I think the best opportunity is Mars for the next decade, for a number of reasons. One of them is consistency with the program in following the water for 15, 20 years, and now we have finally found it, and not just that we found it, but we sampled water in the northern polar regions. So it probably makes sense from a programmatic perspective to continue digging into the polar regions and analyzing the water and looking for life there.

But then there are also other reasons for going to Mars. It is the closest planet to us. It is the one that we understand better in terms of the environment. We know that we need to understand the environment before we start searching for life. That was the lesson learned from Viking. So Mars is the planet that we understand the most after Earth and given this amount of understanding, we should be focusing there.

Then that leads me to the third reason, which is there are places on Earth which are similar to Mars—not exactly the same, but similar, very dry and very cold. We understand how life adapts in these environments on Earth and based on what we have learned, there are not infinite opportunities for life in extremely dry and cold environments. These life forms choose very carefully where they go, which is essentially close to the ice or close to salts. And both features are found on Mars as well.

The strategy of searching for life on Mars would be relatively simple. We can search in very specific niches, and if we cannot find life there, chances are it is nowhere else on the planet.

So I think Mars makes more sense than other places, which we do not understand very well yet. In regards to other locations we do not have examples on Earth that we can base our strategies on.

**CPMcK:** *Okay, great. Inge?*

**Inge ten Kate:** I would pick Mars, as well. I agree with Everett that if we go there we definitely should try to develop something that can go below the surface. I know that it will be very difficult, but at some point we should do it, because on the surface itself there might be a low chance of finding anything.

There is actually a different point that I would like to add here. Instead of only going places with missions to do *in situ* research, we should be doing Earth-based studies as well, focused on microbes that use other mechanisms, elements, or metabolic pathways than life as we currently know it. For example, like the work Dirk is doing, or Felisa Wolfe-Simon.

**ES:** I agree that if we greatly stretch our minds about what life is like on Earth, that could really help our perspective about the rest of the Solar System.

**CPMcK:** ***No one mentioned sample return, and yet in the Mars community there is a stampede toward sample return. So does anybody want to comment on that?***

**PJB:** I would, because I am on a NASA working group that is attempting science definition for the proposed integration of the European Space Agency (ESA) and NASA on a sample return mission, and I am sort of carrying the banner of planetary protection with me on this. So I have been thinking about it a great deal.

Mars sample return is very problematical from the point of view of detection of extant life because of the issues of trying to imagine how any kind of biological or biosignature content is going to stand up under the current scheme of a multi-mission strategy where you are caching samples and then coming along some period later with another spacecraft and grabbing them. Then of course, there is the long transit time back to an Earth receiving facility or lunar receiving facility. This is very problematical for the adequate preservation of anything that we might be able to credibly argue has biological significance by the time it gets through this process.

Now, in my current capacity, I am worrying about the prospect of returning material that might be a potential contamination hazard. But there are severe constraints on this kind of a mission approach, specifically for astrobiology. Now, do I think that a Mars sample return is worth doing for geochemical and geological purposes? Perhaps. It depends on the cost. The idea of grabbing actual samples to study is quite invaluable. But I think that it is not a mission whose primary objective is going to be, or necessarily should be, the astrobiological part of the science that one wishes to do on Mars.

**DSM:** Some of the Viking experiments that were run were stored in a cache for two to four months, and whatever that agent was—whether it was chemical or microbial—that released the gas in the labeled release response, it was not observed anymore after that time. This showed that whatever the cause of activity was, if you store the material for a very long time you might not see anything anymore.

My other point is that there would be quite a bit of public concern about bringing samples back from Mars. I mean, we probably all agree that there would not be very much of danger, but nevertheless we have to be very sensitive to the public perception.

**ITK:** I agree with both of you, but then there is the idea of cross-contamination. Especially with the sample caching idea, if you dump different samples from different locations on top of each other, how can you distinguish between the sample sites? You really need a lot of organic material, I would say, to be convinced that if you get it back on Earth that there has not been some kind of terrestrial contamination.

**PJB:** There are a lot of technical issues: Do we try to actively refrigerate it for X amount of months while we wait for spacecraft number two to arrive and so forth? I feel that in terms of attempting to optimize how much science you get for how many dollars you spend on missions, from the astrobiological point of view, *in situ* measurements made by landed missions are probably the way to go.

That does not mean that I do not support the idea of a Mars sample return, because I have one foot firmly in geology as well as one of my many other feet firmly in biology, so I also care about the geochemistry of the surface. I care about trying to develop some absolute chronology for martian materials and so forth. We do not have the current capability, as far as I understand it, to do *in situ* isotopic fractionation analyses and things of that sort. So there are a lot of purposes that I can see for Mars sample return, but I really do not put my astrobiological interests and those of the community really high on the list of what the science return is likely to be—other than in general, the more we characterize the Mars environment in terms of all of its physical and chemical parameters, the better off we are able to anticipate where we might actually put life detection missions in the future.

**CPMcK:** That brings up another question, Penny, which is: ***What is the right balance between directly searching for life and investigations that focus on the physical environment providing a context for life? Related to that is the question: does the current program strike the right balance?***

**PJB:** I served for about three years on the National Research Council (NRC) Complex Panel and just about every briefing we received from anyone within the planetary programs always included the life question, because it is something that's on everyone's mind, whether they do this kind of science or not.

This question is one that I have struggled with a lot. To scope out the physical and chemical environment is really inextricably bound to the search for life, and it is true that we have focused a great deal on that because, truthfully, it is a lot easier to measure a physical parameter on Mars than it is to, "search for life," because that latter question is so open-ended. We have a very poor constraint set on what we actually mean by the term "life," and searching for biochemistry and macromolecules that look just like those on Earth is not an efficient approach. It is much more challenging to imagine how we would actually design a real life detection mission.

So people are tempted to shy away from coming to grips with that very difficult epistemological question, which is: How do we know we have succeeded if we are anticipating looking for life that might be either reasonably different from us or radically different from us?

This was a dilemma that was not successfully overcome with the Viking missions, as we all know. So we had a certain paradigm that informed those missions about what life would do and how it would behave, and the experiments were all designed to that set of precepts. It was the best that could be done then. I am not sure we could do that much better now because we need a design that is open-ended enough to allow us to really explore, and that open-endedness is really anathema in terms of the way space missions are constructed and controlled. We have a real philosophical and methodological dilemma here about how to push in the direction of greater emphasis on actual life detection missions.

**ES:** I would like to propose a possible compromise position on that.

**CPMcK:** ***Go ahead.***

**ES:** One way of going about this would be to look for chemical or physical signals that we would associate with life rather than necessarily having to look for the biological entities themselves. A way forward on this would be to come up with the methods for determining the rates of reactions that, if we could observe something changing we would need to be in the right environment, where we think something might change. If we could observe a rate of change, especially for an oxidation reduction reaction that we cannot otherwise explain through nonbiological mechanisms, we could at least say that we've found something more likely to be evidence of active life on another planet.

Then, you take your next step from that point in terms of trying to figure out, can you actually find what this thing might be, if it is actually really there and so forth? If we just look for things, biomolecules, etc., it is not going to be as useful as looking for a process. I would particularly advocate looking for rates of oxidation reduction reactions that we cannot otherwise explain.

**PJB:** I think that is a good idea, but the danger I see is in calling such an activity a life detection mission—from the public perception point of view, right?

**ES:** Well, possibly.

**PJB:** Because if you are someone who is not a scientist and you are looking at the news coverage of the latest Mars mission, and it touts itself as a life detection mission …

**ES:** Yes, you might have to call it something subtly different.

**PJB:** So, I think what you suggest is tantalizing, and it might be an interesting transitional type of mission to bridge between the strictly physicochemical characterization missions and life detection, but I think we had better not call it a life detection mission.

**ES:** The problem is, you run the risk of the aftermath of Viking if you say, "We are going to have a life detection mission," and then you do not detect anything.

**PJB:** Yes.

**ES:** Then everyone says, "Oh, well, that is boring," and then it takes two decades to get back to it.

**PJB:** Right, and we do not want to do that again.

**CPMcK:** We are sort of implicitly thinking Mars in this discussion, but for a moment, what if we turned to the outer solar system—Europa, Titan, and Enceladus—and follow that same logic, of just laying out: ***what does that mean for the missions that are now being contemplated, the flagship missions to Europa and possible missions to Titan and Enceladus, in terms of Everett's desired focus on subsurface, and Dirk's comments about Titan at the very start?***

**DSM:** I can say something to that. In regard to Titan and Europa, we do not have anything close to a life detection experiment going to either place. We are really a step short of that. Since the environment and the microenvironment is intrinsically linked to life—and that was a lesson from the Viking mission—we have to understand the environment really well on both Titan and Europa before we can launch programs going more directly towards life detection.

But, both of those moons are really intriguing possibilities. Titan is especially interesting for me, and I know for a couple of other people, because it is so different; if we find life on Titan or beneath Titan, both possibilities are open, it would be biochemically very different. If we actually find life or something very close to it, it would basically imply a different origin of life or protomolecules toward life. This will provide us with an opportunity to learn so much.

**PJB:** Could I add something, Chris, before you move on? We have a couple of bodies in the solar system that are conspicuously "spilling their guts," right?

**CPMcK:** Yes.

**PJB:** Enceladus is obviously the newest one of those two, but Europa is also undergoing extensional and compressional tectonics in its thick icy crust, and this is resulting in what appears to be a regular transport of material from somewhere subsurface and then freezing back out on the surface. This is interesting even with the caveats of the strong radiation environments of the gas giants probably reworking that material deposited on the surface. But nevertheless, it seems to me that targeting … moons that are clearly transporting their interior materials to the exterior so we can actually look at this material, is of great value.

Titan is a wonderful, tantalizing topic, and I am still trying to get my head around geomorphology that looks so similar to Earth's and yet is done with an entirely different solvent system. But I agree that we know so little about Titan, and imagining operating on Titan's surface for very long is pretty daunting. But I think that in the case of Enceladus and Europa, I can imagine missions to those places sooner than I can for Titan that would actually start shedding light on some of these fundamental lifelike processes.

**ES:** I think taking advantage of the behavior of these places, where there are processes that bring interior material to the surface, if we can be as clever about that as possible, that would perhaps get around some of this need to actually get to the subsurface. On Europa, the surface is not a very pleasant place. It is awfully cold and it is an awfully nasty radiation environment, and so, at least based on things we think about in terms of life, that does not seem very pleasant. But the subsurface could be a lovely place to be.

So we are probably not going to fly a drilling rig to Europa, but we could take advantage of the locations where the interior is coming to the surface. If we can figure out what the chronology of that is we can figure out what places have the most recently arrived material.

**CPMcK:** ***Let me follow up on that and ask: is there a logic for trying to get to the plume of Enceladus and look at the organics there? We spend a lot of energy scratching on Mars for organics, and now they are dripping out of Enceladus—why are we not shifting more attention that way? Or should we? What would we look for if we did?***

**ITK:** I would use a completely different approach for Enceladus than for Mars. To Enceladus I first would send an Enceladus-specific exploration mission, either a flyby mission or a mission orbiting Enceladus. I would start with quantifying what is out there and relating that to what we know now about Enceladus and the rest of the Solar System. Then, I would start looking for more biological origins of the organics on Enceladus, for example, and therefore we need to develop more specific life-detection instruments.

On Mars we are already at the phase where we need these kinds of life-detection instruments, especially since none of the instruments right now on Mars are directly looking for life. There is one instrument going next year to look for organics again and one in a few years from now, and that is about it. So why not focus on developing more life-detection instruments—whether for proof of life or direct life?

And then build upon the Mars heritage to develop instruments to further characterize other planets.

**DSM:** In this regard, actually, the instrument package that comes closest to that would have been part of the ExoMars instrument suite. And based on budget problems, this is now cut. So that gives scientists real frustration, because that was a first step to looking for life or searching for life.

**ITK:** Yes, I fully agree with you.

**CPMcK:** ***I wanted to go back to the question of the difference between searching for the physical environment and searching directly for life. Alfonso, you had argued that you already can identify places on Mars where we could search for life. Would I be interpreting that correctly to say that you think that we need to shift the focus there from understanding the physical environment more towards life detection specifically?***

**AFD:** Basically what we know from analog environments on Earth is that life is not obvious at large scales in extreme hot deserts and cold deserts. It is not obvious in the large scale, but it is obvious in specific niches. We just need to look at the right place, and then it is easy to see if you know where you are looking.

So I think the same might apply to Mars, that if we know what to look for, if it is there, we are going to see it. It is going to be evident. And if it is not there, chances are that it is not anywhere else on the planet. So at this point, I do not think we will look at any more information from the chemical environment from the meteorology or from the climate. I do not think that we will get any more information that is going to help narrow down the number of environments we should be looking for life. It comes down to mostly ice and probably salty places. We know where they are—they have been mapped and they have been characterized.

So I think it is justified to move towards a more aggressive approach and search for evidence of extant life, microbes that are active today or were active 10 million years ago, in the recent past. I think that should be our first move.

Then if we do not find this type of life in these environments, then we can move to the next step, search for past life, and then we would probably go to other places, and so on. Then if we do not find any of those, past life or active life, then I would say we should be thinking about sample return. Then it might be safe to bring samples back, and we might learn about other aspects of the planet. But I think right now, we are starting from the roof. We want to bring samples back most likely to search for past life, looking at ancient sediments from the equator and sample dry sediments.

Chances are that we find something that looks like life, just like we find on Earth in very ancient sediments, but that is not going to solve the question. We would still have to answer the questions, of: We found something that looks like there was life on Mars, but is there life on Mars now? Then we will have to go back to the planet again, search for life in those places. So I think we are starting from the roof with the sample return.

**CPMcK:** ***Your comments, Alfonso, are directed mostly at the surface. So I want to follow up on Everett's focus on the subsurface and say, what is the path forward for Mars in terms of understanding the subsurface, and are we at even step one in understanding the subsurface right now? And where is the balance in terms of searching for life, understanding the subsurface, or even making any progress at all?***

**PJB:** The current thinking about the Mars sample return, for example, is that ExoMars will have the capability for a two-meter drilling capacity, which is essentially the surface. It is barely below the surface enough to matter in terms of looking at a significantly different potential habitat although it does penetrate below the immediate UV affected surface. So the current plans for the Mars sample return really have no actual subsurface component per se.

When you look at the zone of Mars that is likely to be affected in terms of the ionizing radiation environment, it is probably the upper 10 meters that is highly radioactive, or highly affected by ionizing radiation in terms of propagation of daughter products. This is particularly true in the regolith, which has much less shielding capability than bedrock. Recently, I have been looking into it in terms of salt, andesite and other volcanic materials, and truthfully, until you are greater than about 4–10 meters down, you are not even out of that surface high-radiation environment.

So, based on the work that we do in caves on Earth and so forth, anything short of at least a few tens of meters is not reaching the subsurface in any astrobiologically meaningful way. The notion of developing methodologies to do remote drilling on nonhuman tended missions is pretty daunting, because if you have been on any kind of drilling operation on Earth, you know the difficulties. It is a very labor-intensive operation, and imagining trying to do that robotically really boggles my mind.

I have sat in on my share of panels and workshops on this issue of drilling into Mars, and a lot of the solutions that are being proposed for missions … we are really pretty far away from being able to implement those. So this notion of drilling into the subsurface or somehow accessing it through natural caves, which is all near and dear to my heart, but imagining that over the time frame of the next decade is pretty difficult to do. However, I think we should certainly be starting more aggressively to develop the capability.

One of the most hopeful signs that I've seen is increasing attention to natural openings into the martian surface by way of, what appear to be, lava tubes, skylights, and so forth.

**ES:** It would be pretty appealing if you got down in there and there was some ice and maybe some liquid water around or something.

**PJB:** Oh, absolutely. What we have been proposing for more than a decade is that there will be collapsed lava tube segments on Mars that probably are going to be very "juicy."

**ES:** Yeah.

**PJB:** Maybe they will contain trapped volatiles or at least particles that might shed light on Mars' climate, perhaps repositories for organic materials, and maybe even bug bodies. Who knows? But they would certainly be little time capsules into which a mission could drill and actually punch through into something that would have a higher than average, I believe, chance of having interesting contents. But one would have to be able to find those, identify them, and then persuade engineers that you could safely land a spacecraft there. Those are the hurdles.

**CPMcK:** ***Everett, do you have any thoughts about how we advance our understanding of the physical environment in the subsurface, providing a context for doing what you mentioned, which is a search of life underground?***

**ES:** I think finding natural openings into the subsurface and building relatively small robotic devices that could get into such places and explore around and come back and tell us what they found is a way to make some serious progress. But complexities of drilling are, as has been mentioned, bad enough when you are on the Earth. And you always are, with any drilling operation, punching a big hole into the thing you are trying to study, and you are disrupting the system by the very act of doing that.

So I think finding ways into the surface that already exist could be very productive.

**CPMcK:** ***Could remote sensing tools be of any use? Have the radars that people have flown on the recent missions given us any better insight into the subsurface?***

**ES:** I have not heard much about subsurface structure being revealed by these methods. Maybe it is out there and I am just not aware of it. We have a lot of evidence telling us that there is a potential for liquid water below the surface. I would think you would use that kind of mapping as one piece of information to guide where to look to maximize your chances of finding useful things.

**PJB:** Landed ground-based radar missions would be pretty sweet. You know, we can detect subsurface cavities, with reasonable resolution, down to 100 meters or more in depth.

**DSM:** However, if you have a radar from orbit, the most you can do is really a couple of tens of meters. It depends on the moisture content. For example, the most that was viewable in the Sahara Desert from an orbital view was about 30 meters.

**ITK:** What about the radar that is flying on the Mars Express? It would look down to a few kilometers. That showed some basins under the surface, but that was, I think, a larger scale than we are talking about right now.

**CPMcK:** ***It seems like there are these radars returning data, but I was expecting much more discussion of Mars underground from these radar results. Maybe it is just that the data is so difficult to read that they are still working at it, thoughts?***

**PJB:** I think that is true, Chris, and I also think that we probably are suffering from a relative lack of interest in the subsurface features—that we are all interested in—from the teams who are actually doing that. I think there is room for interacting with those people and drawing their attention to the scale of the things that we are interested in, too.

**CPMcK:** For our last point, I would like to discuss the make up of the Decadal Survey. The survey is set up to mirror the NASA headquarters' alignment and structure, and one sometimes wonders whether it is bureaucratically set up in a way that does not really get effective community input? Another activity that is similar to the Decadal Survey is the Astrobiology Road Map, that is generated and promulgated by the NASA Astrobiology Institute. ***So my question is: Do these processes effectively capture the thought of the community? And if not, is there any way we can improve or get more broad-based community input?***

**DSM:** Well, the one criticism I have of astrobiology programmatically being part of the planetary program at NASA headquarters is that a big part of astrobiology is also origin of life and extreme environment research, and that those large chunks of astrobiology are not directly related to planetary sciences. But I do not have any, really, suggestion how to do it better because I am not as familiar with this kind of setup.

In regard to the Astrobiology Road Map, I think that the Road Map is pretty helpful in general and that it being under the mantle of the NAI seems to be fine to me, too.

**CPMcK:** One thing to think about is, this teleconference is part of this new astrobiology society activity. Maybe there is a role for this independent organization that could not be filled otherwise ... Sometimes astrobiology gets accused of being nothing but a creature of NASA.

**PJB:** I actually have pretty strong feelings about the way things are currently set up and what is wrong with it. My dilemma is that I do not have what I think is a very good alternative. One could argue for something really radical, like splitting off astrobiology into its own directorate for example, to sever it out from under the planetary components and have it be its own entity at some organizational level.

But I see distinct dangers in that, because really our access to planetary missions is a critical component for the future development of astrobiology, as well as ground-based stuff, as Dirk was saying. So what I worry about is if astrobiology goes off on its own that it will then be even more difficult to find a way to have astrobiology make its way into missions and piggy-back on primarily non-astrobiological missions.

I think that the way the NASA Astrobiology Institute (NAI) is conducted is very exclusionary ... This may be unintentional, but it really is a divisive thing within the astrobiology community. So the current astrobiological setup is really pretty non-ideal for integrating the broader astrobiology community.

The Road Map itself was a worthy thing, and it actually did have a lot of outreach, and I participated in that process. But I also see that there is a lack of penetration of a lot of the basic precepts that were put forward in that Road Map. There is a lack of penetration of those ideas and priorities into the planetary community at large, and so that is another barrier to advancing the causes of astrobiology.

The third difficulty is that some of the goals and needs of astrobiological science is in opposition, really, to what other parts of the planetary community need for individual missions. So there is this inbuilt tension between the astrobiological science and its needs and those of other planetary sciences. I do not have a really great solution at this point, but I would like to see a higher profile, really, for the Road Map activity.

**CPMcK:** *Everett, do you want to weigh in on the same topic?*

**ES:** From my perspective, there is considerable effort on many people's parts to integrate biological and astrobiological things into some mission planning, and I think there perhaps could be more of that. But it seems to me to often be a natural part of some of the mission planning. So I guess I may have a less pessimistic view of it. I think the idea of an astrobiology society that actually did something like put people together to contemplate alternatives to these things could be a very serious contribution.

**CPMcK:** *Alfonso?*

**AFD:** I guess I also have strong feelings, like Penny, but I will refrain some of those. I just wanted to point out a couple of things. One of them is, to your question of if they do a good job at headquarters at reaching the community, I think it was very revealing what happened in AbSciCon when Steve Squyres, the chair of the NRS Planetary Science Decadal Survey, presented the summary of the survey. One of the major points that he stressed was that the survey would not include *in situ* life detection on Mars. Another thing, he emphasized was that the survey was based on input from the community. It turns out that most people in AbSciCon were surprised by this. Most people in the astrobiology community were surprised that *in situ* search of extant life was not considered in the Decadal Survey.

So I do not know why that is the case. Clearly there was a miscommunication at some point. In two weeks, we gathered 150 signatures to support *in situ* life detection, and that document was sent to the Decadal Survey. I do not know what the reason for that is.

One possibility is that there is a large percentage of the community doing astrobiology who are not part of NASA—universities and the like—and they do not get access to that information about the Decadal Survey. Many people I talked to in astrobiology did not know what the Decadal Survey was. They did not even know it existed. So people are doing astrobiology, but they are not aware of the program, I think.

Then the other issue I see is that there is a completely different story to search for life on Mars than to search for the origin of life on Earth. Everything gets placed in the same box, astrobiology: searching for the origins of life, understanding the evolution of life, searching for life elsewhere in the Solar System. I think we are very quick to link these things together, and so it is very hard to manage a program, but it is so broad in scope.

If you look at the NASA astrobiology program, the main topics of interest are the prebiotic life, the origin of life, evolution of life, how life becomes complex and searching for life elsewhere. So searching for life in the Solar System is one-sixth of what NASA's astrobiology program is about.

So maybe we should be splitting some of the concepts that we put in the same box, like searching for life in the Solar System and understanding the origin of life. Maybe that should be considered in different terms and different approaches.

**CPMcK:** *Okay, great. Inge?*

**ITK:** Well, to go back a little bit more to your original question, I was actually reading the old Planetary Decadal from 2002, and I was surprised how under-highlighted astrobiology was in that framework. I mean, some of the sections did have a link to astrobiology, but not all, and at some point there were a few pages that mentioned astrobiology, but it did not have its own chapter, and you really had to look thoroughly through the text to find astrobiology-related material.

So what I am really hoping from this Decadal Survey is that there is actually a stronger section on astrobiology. It definitely does have a part in all of the different programs, but it might be a good idea to have a better emphasis on astrobiology by adding a summary of the different astrobiology goals and how they link to the different programs, after the other programs have been described. That would put a better emphasis on astrobiology than was done in the last Decadal Survey, from my point of view.

With respect to what Alfonso just mentioned, about the splitting of concepts concerning the search for life in the Solar System and understanding the origin of life, I don't agree with that. I think origin of life and life elsewhere are very closely intertwined and that new findings in one subject will only benefit the other. I'm afraid that by splitting those two subjects the whole interdisciplinary idea behind astrobiology is defeated again and everybody will return to working in their own little box.